

death coming, said to his servant, "Mind what I say—I am going to die. When I am dead, but not till then, go to Lord George Cavendish and tell him—go." There are Lavoisier and Dumas, at once men of action in many departments of public life and men of thought, men of the world, and men of science; and there is Faraday, the humblest, the simplest, the most accurate, and the most original, of men.

The book abounds in examples of the dangers, the difficulties, and the triumphs, of the scientific method. We see Priestley on the verge of great discoveries regarding the composition of water and metallic calces, but held back by his devotion to that idol of the theatre, *phlogiston*. We see Boyle meeting the statement of Linus, that the mercury in the Torricellian vacuum is upheld by a kind of internal cord, by measuring the decrease in the volume of a portion of air caused by increasing the pressure on that air, and so arriving at the fundamental statement regarding the relation of gaseous volumes to pressure which is known as *Boyle's law*.

We are able, too, to compare the thoroughness and methodical application of the Germans, as shown in the work and lives of Wöhler and Kopp, with the brilliancy and sweep of the French naturalists Lavoisier and Dumas, and also with the stubborn perseverance and imaginative grasp of such English men of science as Dalton, Graham, Faraday, and Boyle.

The comparison of the two great atomists, Dalton and Graham, made on pp. 225-6, is succinct and suggestive. But I cannot agree with the author in his apparent approval of Henry's statement that imagination had no part in the discoveries of Dalton. I am sure that everyone who has attempted to teach the essentials of the atomic theory must be convinced that without vivid imagination it is impossible to gain any firm hold of this great conception. The whole of Dr. Thorpe's book, indeed, may be taken as a complete refutation of the vulgar mistake that the man of science has no need of imagination.

One may be repelled by the solitary, non-human life of Cavendish; but the character of Faraday, as set forth in Dr. Thorpe's sketch, must attract every reader. The accounts of the first meetings of Wöhler with Berzelius, and Dumas with Humboldt, given in the words of Wöhler and Dumas themselves, show that to men of science too comes sometimes the glow of hero-worship.

Who but a Frenchman would express his joy at a new discovery made in the laboratory by seizing the discoverer and waltzing with him round the benches! This was Gay-Lussac's method. I have been present when the arrival of a few tarry, evil smelling, drops of a long-wished for compound has been hailed with shouts and songs; but the translation of the emotions of a chemist into the poetry of movement is a higher and more inspiring flight.

The criticism given by Dr. Thorpe of the conception that underlies the hypothesis of Prout is one that chemists would do well to remember. In its original form, Prout's hypothesis suggested that the atoms of the elements are collocations of atoms of hydrogen, and that, therefore, the atomic weights of the elements are whole multiples of the

atomic weight of hydrogen. Dr. Thorpe very justly remarks of the work of Stas (p. 229):

"It may be that it demolished Prout's hypothesis in its original form, but it has not touched the wider question; indeed it is very doubtful whether the wider question is capable of being reached by direct experiments of the nature of those of Stas, unless the weight of the common atom is some very considerable fraction, say one-half or one-fourth, of that of the hydrogen atom."

It seems to me that chemists have been very ready to forget "the wider question" that underlies the hypothesis of Prout. That wider question is: Are the elementary atoms collocations of different numbers of atoms of one fundamental kind of matter? And Prof. Thorpe does well to point out that this question cannot be answered by measurements of the relative weights of the atoms of the kinds of matter that we call elements. If the best established values for the atomic weights are multiplied by one hundred, all the values will be expressed in whole numbers; it is then only necessary to assume that the atom of the fundamental matter is one hundred times lighter than the atom of hydrogen, and, if the wider question underlying Prout's hypothesis can be answered by measurements of atomic weights, that question is answered. The solution of the world-old enigma of the unity of matter must be sought for by other methods.

On p. 154 the author quotes from Faraday, who, when writing to a scientific man at variance with another, remarked:

"These polemics of the scientific world are very unfortunate things; they form the great stain to which the beautiful edifice of scientific truth is subject. Are they inevitable?"

Would it not have been better had Dr. Thorpe, remembering this wise remark made by Faraday, who was the perfect representative of the true scientific student of nature, omitted from his collected essays that entitled "Priestley, Cavendish, Lavoisier, and *La Révolution Chimique*"? This essay jars somewhat. It is not in harmony with the others; it is not concerned with matters of universal interest; it revives a controversy that surely had better been left in forgetfulness. For is it of much importance to determine whether Lavoisier was or was not wholly indebted to Priestley for the fact that *red precipitate*, when heated, gives off a gas wherein a taper burns brilliantly?

Lavoisier can afford to make full acknowledgment of indebtedness to Priestley. Priestley discovered dephlogisticated air; Lavoisier gave oxygen to science.

M. M. PATTISON MUIR.

THE ORIGIN OF GLACIAL DRIFTS.

The Canadian Ice-Age. By Sir J. William Dawson, C.M.G., F.R.S. (Montreal: William V. Dawson. New York and London: The Scientific Publishing Company, 1893.)

IT is continually brought to the notice of geologists that the most recent period in the long history of the earth is also that which excites the greatest controversy. We can deal complacently with earth-movements, mountain-thrusts, and submergences of half a

continent, so long as the organisms affected by these occurrences are less specialised mammals than ourselves; but we find it hard to believe in great physical or climatic changes within the limits of our own written or unwritten history. Moreover, our knowledge of the post-Pliocene period is burdened with an excess of detail; and broad and sweeping generalisations seem at present out of the question. And, if we go one step further, we may fairly attribute our friendly agreement with regard to the conditions of the older periods to our ignorance rather than to our information.

Sir William Dawson, in the present work, summarises several previous papers of his own, just as M. Gaudry's detailed memoirs were summarised for general use in "Les Ancêtres de nos Animaux." This handy paper-bound volume deals strictly with Canada, and is in no way a "Theory of the Earth." It is moderate in tone, and forms a serious plea for a rational treatment of the glacial epoch. Whatever caused the cold conditions in the northern hemisphere, or in parts of the northern hemisphere, it is pointed out that the land-ice in Canada radiated from two local centres, and not from the hypothetical ice-cap at the pole. Readers of NATURE will remember the evidence brought forward by Dr. G. M. Dawson as to the "Laurentide" centre of glaciation on the east and the "Cordilleran" centre on the west (NATURE, vol. xlii. p. 650). The conditions maintained by Sir W. Dawson as most favourable to the development of glaciers are high masses of land in proximity to cold seas; and, as he properly points out, these conditions still prevail in North America to a greater extent than in North Europe. They prevail, moreover, in Greenland, but not in Grinnell Land, to cite two closely neighbouring areas.

It will be clear, then, that Sir W. Dawson urges that differential earth-movement was the main factor in the production of Canadian glaciation. The evidence of marine shells in the drift, of the bones of whales, of the character of the deposits themselves, all points to the existence of wide areas of submergence. With regard, for example, to the Cordilleran centre in British Columbia, our author writes:—

"The conditions were combined of a high mountain chain with the Pacific on the west, and the then submerged area of the great plains on the east, affording next to Greenland the grandest gathering-ground for snow and ice that the northern hemisphere has seen."

Of recent years it has been far too generally assumed that we have to picture the glaciers of the ice-age moving across the features of the country as we at present know them. The views of Prof. Suess with regard to earth-movements in the historic period are perhaps only fair criticism of somewhat hasty observations; but, in face of the extraordinary evidence of post-Pliocene upheavals, it is at least irrational to believe that these terminated with man's appearance on the globe. Many English "glacialists" accept a recent submergence of their country to a depth of 500 feet, and yet postulate the most catastrophic occurrences to account for marine beds at twice that height above the sea. Yet we now have, in addition to the old Lyellian instances, such as the Astian or even later beds in Sicily, which are elevated some 3000 feet,

evidence given us by Prof. Andrew Lawson of a post-Pliocene uplift of the continental coast of California to heights of from 800 to 1500 feet; and Sir W. Dawson's requirements to explain the distribution of the Canadian drift are such as will seem moderate and natural to every rational uniformitarian.

On p. 111 of the present work, the author discusses the possibility of distinguishing striations produced by the "huge ice-islands" in shallow seas from the deeper and firmer markings of true glacier-ice. Granted the submergence, which in itself assists in the formation of snow and ice, the phenomena of the distribution of boulders receive at once their simplest explanation; and in chapter v. the local details of the drifts are taken, area by area, into consideration. Our own British islands must similarly be discussed area by area. Because it seems probable that Scotland in the glacial epoch was a local Greenland, there is no reason why England should also have been lifted above the sea. The evidence accumulating in Ireland goes far in favour of a long submergence of that country, with the production of an archipelago of picturesque and snow-capped islands. Hence it is that we may welcome Sir W. Dawson's summary of results in Canada as a reminder that land-ice and enormous terminal moraines are not to be left in undisputed possession of the field. We can even sympathise with him in his final sense of irritation, when he charges some glacialists with "misunderstanding or misrepresenting the glacial work now going on in the arctic and boreal regions." "These are grave accusations," he continues, "but I find none of the memoirs or other writings of the current school of glacialists free from such errors; and I think it is time that reasonable men should discountenance these misrepresentations, and adopt more moderate and rational views."

Of course Sir W. Dawson cannot resist the temptation of stating as "an inevitable conclusion" (p. 289) "that the origin of specific types is quite distinct from varietal modification"; but this is a cheerful side-thrust, as it were, in a work on quite another subject. On p. 36 the use of "Neozoic" as equivalent to "Tertiary" seems unusual; and on p. 51 there is a sentence on the origin of fiords, quoted from an earlier paper by the author, which describes them as "often evidences of the action of the waves." They may have nothing to do with glacial excavation, but still less can they be regarded as products of marine erosion, unless the author confines himself to the cases that he has specially examined in Nova Scotia.

G. A. J. C.

OUR BOOK SHELF.

Grundzüge einer Entwicklungsgeschichte der Pflanzenwelt Mitteleuropas seit dem Ausgang der Tertiärzeit. Von Dr. August Schulz. (Jena: Gustav Fischer, 1894. 8vo. pp. 206.) (Outlines of a History of the Development of the Flora of Central Europe since the close of the Tertiary Period.)

IN a preliminary note by the author, we are informed that this dissertation is an extract from a more extended essay on the vegetation of Central Europe, which present circumstances prevent him from publishing in full. The title cited above is really that of the first chapter of the work only. A second deals with the